Responses to the Reviewer 2 (comments in *italics*, responses in regular).

The authors present a study of microphysical choices on the macroscopic impact on convective clouds in a set of large-eddy simulations applying warm-rain bin micro- physics. The impact of the employed collision kernel as well as to the prescribed CCN concentration is investigated. On the cloud scale the study documents a change in the condensate offloading and cloud droplet evaporation. On the scale of the macro- scopic cloud field impacts are minor. In addition, the authors discuss if effects of cloud turbulence on the formation of drizzle or rain could be observable from space.

While the work presents some interesting results, it is difficult to read the paper as a self-contained study. In many parts it refers to the previous study of Wyszogrodzki et al. (2013) and the reader gets the impression, that the current paper is an extension of the previous work.

Moreover, the results presented show some deficiencies in originality. A comparison between the traditional gravitational collision kernel and the turbulent kernel has been presented in the Wyszogrodzki et al. (2013) study already. Concerning the question on the macroscopic impacts of chosen CCN and the collision kernel, the authors would need to work out more clearly, where they distance themselves from e.g. the Stevens and Seifert (2008) study and what the new content of the current study is.

In addition, the study could benefit from being more precise at certain places. E.g. in those places where cloud water evaporation is brought forward as a mechanism, the evaporation is never shown explicitly. theta_d and theta_e are shown, but if the authors presented output from the microphysics routine indicating the evaporation rates (or recalculated evaporation rates from the output fields) this would strengthen your results.

We appreciate the reviewer's comments. Indeed, current paper is a follow-up to Wyszogrodzki et al. (2013): it provides further support for the other paper suggestion (i.e., the condensate off-loading leading to changes of the cloud top distribution) and it applies the simulation data to see if it would be possible to use remote sensing (e.g., from satellites as in Suzuki et al. 2013) to support the impact of turbulent collisions on drizzle/rain development.

As for the first part of the paper, our goal was to go beyond just assessing the impact of turbulent collisions (which was the goal of Wyszogrodzki et al.) and to document the role of two contrasting effects: drizzle/rain development and cloud droplet evaporation near cloud edges. The latter subject was not considered in the Stevens and Seifert paper and they did not use the bin microphysics model (their bulk model still applied saturation adjustment and was not able to look at the effects of droplet evaporation). As for the droplet evaporation, this was analyzed in depth in Xue and Feingold (2006) and we do not think we need to repeat such an analysis. Just showing the outcome (i.e., smaller cloud fractions for cloud fields featuring small cloud droplets) is sufficient. Some of these aspects are now discussed in the revised discussion.

Overall, we introduced many revisions to the manuscript following the spirit of the above comments to better expose specific aspects of our study.

General Comments:

1. I suggest to include the word "Macroscopic" into the title ("Macroscopic impacts of cloud and precipitation processes on maritime shallow convection..."). This would emphasize the focus of the current study and contrast it to the Wyszogrodzki et al. (2013) study.

We added "macroscopic" to the title.

2. Include some more explanations to important aspects of the study in order to make the current study more independent of the other study. In section 2 (numerical model and setup) indicate the domain size again. I also suggest including a figure showing rain rate and/or cloud water content once more. Otherwise the discussion is hard to follow if the reader is not familiar with the Wyszogrodzki et al. (2013) study.

We added more information on the model setup and a table showing 6 hr domain-averaged rain accumulations as well as the cloud cover in all simulations.

3. In most cases only the N30 and N240 cases are discussed, as these show the most extreme results. As you state, the other cases are situated somewhere between these extremes. You could consider discussing only these two simulations from the beginning and to add a section termed something like "additional studies" in the end which explains the intermediate simulations.

We feel this comment represents a personal choice of how the discussion can be structured. We prefer to have it as in the original paper, rather than splitting it into separate discussions, that is, first contrasting N30 and N240, and then adding the discussion of N60 and N120 in a separate section.

4. The authors investigate vertical profiles of potential temperature and water vapor mixing ratio to show the macroscopic impacts of the microphysical choices. It would be nice to see some more explanations later on in the paper, why the profiles look different and how this is linked to cloud-droplet evaporation and condensate off-loading. The temperature profiles in Figure 1 show a difference of ~0.8K at a height of 1.7 km, where cloud fraction is roughly 2-5% (Figure 7). This would correspond to a difference in the evaporated water of 0.016-0.0064 kg/kg. Is this about reasonable?

We decided to drop this aspect from the revised paper per specific recommendation of the Reviewer 1 (with which we tend to agree).

5. CAPE is used as a measure for the macroscopic impacts on clouds, but deeper clouds for higher values of CAPE are not found. Maybe CAPE is not a good predictor for cloud-top height in this case. As you show cloud depth is to a considerably larger degree controlled by entrainment/detrainment, their size, and thereby degree of organization than to the profile. Add some more discussion on the usefulness of CAPE for shallow clouds.

We agree with this comment and decided to remove the subject from the manuscript. The analysis presented in the original manuscript shows that CAPE (either adiabatic or pseudo-adiabatic) is indeed not the best predictor of cloud dynamics in situations where precipitating and non-precipitating cloud populations are to be contrasted.

6. In section 4 the authors reason about the utilization of satellite data to confirm the impact of small-scale turbulence on the warm-rain initiation. The purpose of this section is not entirely clear to me. The collision kernel is a parameterization, where one of the two formulations is reproducing nature more adequately than the other one. In nature, only one realization of the experiment is performed and no second dataset available for comparison. Work out more clearly, what you exactly want to compare with which data. Is the aim to distinguish regimes in which turbulence collision is more important than in other cases? Or is the aim to verify the effect by comparing modeling data to observation data?

Section 4 tries to answer the question is satellite observations can be used in support of the impact of the small-scale turbulence on warm rain development in shallow convective clouds. We compare cloud field statistics from GRAV and TURB simulations treating them as model realizations of natural cloud fields assuming that the rain develops through gravitational collision/coalescence either excluding or including enhancement due to small-scale turbulence. In other words, this analysis can be treated as a feasibility study for the assessment of the role of cloud turbulence in drizzle/rain development applying satellite data.

Specific Comments

Page 19840: 3-5: the presented study shows the opposite, namely deeper clouds for larger cloud droplets due to the condensate-offloading mechanism. Discuss these results in the context of your work once more in the discussion section.

Added.

Page 19842: include the domain size into the description of the setup Page 19842-19843: include a figure showing the rain rates.

The discussion of the model setup was expanded and the table with 6-hr rain accumulations was added.

Page 19843: why are the theta profiles different? If you cannot answer the question here, come back to it later.

This section is no longer included in the revised manuscript.

Page 19844, Line 22-24: The sentence "the cloud top height ..." is not necessary to understand the current study and can be removed.

Removed as suggested.

Page 19844: concerning the calculation of the cloud-top height: one could also define one single cloud-top height per cloud, either the maximum value for each cloud or some different measure. Thereby you could also circumvent the problem of the increased number of columns from rain which is stated on page 19845, lines 16-18.

Yes, we agree that the cloud top can be defined as a single number per cloud (which would be more complicated and may lead to some ambiguity for clouds featuring separate turrets). The fact that the number of column increases with the increase of CCN concentration is related to the cloud fraction and cloud cover (the latter was added in the revision and included in the table).

Page 19845, Line 15, "the former. . ." explain the dynamical effect once more and the condensate-offloading mechanisms. Any reader not familiar with the Wyszogrodzki et al. (2013) may have difficulties following the arguing.

Added.

Page 19846: in my opinion some of the four groups are ill-defined and may lead to misleading results. I agree with the definition of "cloud updrafts", whereas both "cloud- edge downdrafts" and "ascending strongly diluted volumes" could also be the signature of gravity waves in the cloud-free environment and do not necessarily need to be connected to clouds.

We do not agree. Gravity waves near clouds will have significantly smaller absolute vertical velocities than 1 m/s taken as the threshold in the analysis.

Page 19864: it would help the reader to understand the theta_e theta_d plots more easily if you added a short description on what you would expect to see in these diagrams.

We added couple sentences to the discussion following the spirit of the reviewer's comments.

Page 19864, Line 25-26: volumes with high values of theta_e could also be moistened by evaporation from rain from above and thereby increase their theta_e value. It seems a bit arbitrary to me to restrict high theta_e volumes to originating from the surface. Show more evidence here.

We do not think this is a valid point because evaporation also leads to the reduction of the temperature and the two effects offset each other almost exactly.

Page 19847, line 3-4: what about the other differences between the simulations? e.g. in Fig. 4, the scatterplot for w>1m/s, qc<0.1kg/kg: there are a number of points with relatively low theta_e but considerable positive buoyancy? Which processes create positive buoyancy in these points?

We do not think trying to explain every detail in these figures is needed. The key point is that the scatterplots are similar between various simulations for these four groups of points.

Page 19847, fourth paragraph: above, the authors discuss the impact of varied CCN, then they quickly jump into differences between GRAV and TURB. For any reader who has not read the WGWA13 study it would be beneficial if the main results from the study concerning the histogram structure was explained.

We are not sure how to address this comment. We hope that the added figure and text revisions are satisfactory.

Page 19848: In addition to showing the profiles of cloud fraction, it would be useful to have a plot showing the vertical rain water distribution.

We are not sure what such an analysis would bring to the manuscript. Evolution of cloud and rain water mixing ratios was shown in Wyszogrodzki et al. (2013).

Page 19848, section 3.4: there is large overlap between the profile from the individual time steps, in some cases the variation in time is larger than the differences between the simulations. Even though you comment on the importance of the life-cycle of the clouds, it would be good to have another comment on the issue in the context of the figure.

We added a comment (in the text that this comment applies to and in the discussion) that using the piggybacking methodology would allow significantly more confident assessment.

Page 19849: section 3.5: the entire section is very qualitative and adds very little to the paper. I suggest removing the section but keeping the sentence "Although small-scale ...".

We revised this paragraph. In particular, we added a reference to the piggybacking methodology that allows assessing the impact with high accuracy (Grabowski 2014a,b).

Page 19854, line 23-25: this sentence is hard to understand. Explain more clearly, why these effects explain the differences in cloud-top height distribution.

This sentence has been rewritten.

Page 19855 second paragraph-19856, first paragraph. The authors hypothesize on the impacts of cloud and precipitation processes on the lifetime effect but do not back up any of the statements by data. I suggest to remove the entire paragraph.

We do not agree and the paragraph remains as part of the manuscript. The idea here is to point out that looking at the cloud lifecycle effects (as done by several authors in the past) brings little for the clouds-in-climate problem from the point of view of shallow convection.

Page 19858, line 7-9: a variable that increases both with the cloud depth and droplet size will still not yield a unique answer. One would need two separate variables, one sensitive to cloud depth, the other one sensitive to droplet size.

The discussion here concerns specifically the optical depth that can be retrieved from the satellite observations. We are sure the reviewer agrees with our argument. Additional variables (e.g., the physical depth of a cloud) can be easily obtained from satellite observations.

Technical Comments

We corrected all the technical problems listed below and appreciate the reviewer's effort to list all of them.

Page 19839: line 16: replace "contributions" by "contribution"

Page 19839: line 22: replace "a question" by "the question"

Page 19840: line 24: replace "development" by "the development"

Page 19841: line 20: replace "the second question" by "to the second question"

Page 19844, line 14: replace "terminate in" by "terminate in the"

Page 19846, line 4: replace "by next three figures" by "by the next three figures"

Page 19847, line 29: replace "need" by "needs"

Page 19848, line 13: replace "Figure 6" by "Figure 7"

Page 19848, line 15: insert "the" before "NOCOAL"

Page 19848, line 22: replace the slash between drizzle/rain with "or", otherwise it is confusing with the other usage of dashes.

Page 19848, line 24: insert "the" before "cloud layer".

Page 19848, line 25: insert "a" before "strong inversion".

Page 19851, line 1: insert "the" before "frequency"

Page 19851, line 4: insert "the" before "Frequency"

Page 19851, line 28: insert "the" before "cloud lifecycle"

Page 19852, line 21: replace "van Zantem" by "van Zanten"

Page 19852, line 24: insert "the" before "onset"

Page 19852, line 27: insert "the" before "evolution"

Page 19853, line 10: insert "the" before "earlier formation"

Page 19853, line 14. insert "the" before "upper parts"

Page 19855, line 17: insert "the" before "turbulent kernel"

Page 19857, line 23: insert "a" before "limited set".

References:

Grabowski, W. W., 2014a: Extracting microphysical impacts in large eddy simulations of shallow convection. *J. Atmos. Sci*. (available on EOR).

Grabowski W. W., 2014b: Untangling microphysical impacts on deep convection applying a novel modeling methodology *J. Atmos. Sci*. (submitted).